Anxiety and phantasy in the field: The position of the unconscious in ethnographic research

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Abstract

This article contributes to the geographical literature on reflexivity by asking what it means to take the researcher's unconscious seriously in ethnographic research, and proposes psychoanalysis as a theoretical and methodological resource for researching the unconscious dimensions of fieldwork. I begin by describing three moments from my fieldwork with panhandlers and drug users that evince the operation of the unconscious. I then review psychoanalytic work in the social sciences where the researcher becomes the object of analysis and situate the debate on psychoanalytic methodology as an extension of earlier work on reflexivity by feminist geographers. I outline three methods for investigating the unconscious dimensions of fieldwork: analysis, supervision, and case consultation. Summarizing my experiments with these methods, I discuss: the discovery that key elements of my research were inextricably connected to my own anxieties as a researcher, how analysis of a dream from early in the fieldwork revealed phantasies rooted in childhood and a profoundly ambivalent relationship to my informants, and I propose a dialectical method for incorporating the revelations of psychoanalytic reflexivity into research. I conclude by discussing some of the possibilities and consequences of taking the unconscious dimensions of fieldwork seriously.

"The most novel thing about psychoanalysis is not psychoanalytic theory, but the methodological position that the principle task of behavioral science is the analysis of man's [sic] conception of himself."

Georges Devereux, From anxiety to method in the behavioral sciences, 1967, page 3.

"It is good form to introduce a work in psychology with a statement of its methodological point of view. I shall be derelict. I leave methods to the botanist and mathematician. There is a point at which methods devour themselves."

Franz Fanon, Black skin, white masks, 1967, page 12.

Three moments in the field

1.

It's Friday night and I'm going out for drinks with my new advisor. I've just started the Ph.D. and I'm talking excitedly about my project, which will be an ethnography of drug-using panhandlers in Vancouver's Downtown Eastside. He's new to the city, so I'm playing the local, taking him to a bar I know in the neighbourhood. Walking at night, I usually avoid the most intense blocks of the drug scene, but that night I decide to take him straight down Hastings, to show him this place that looms so large in the city's imagination. (Looking back, it's obvious that I was showing off a bit: trying to show him how comfortable I was in the neighbourhood, how I could easily navigate this place that was spoken about with such fear. As it turned out, that night reminded me just how much the neighbourhood could still unnerve me.) As we round the corner at Columbia and Hastings, it feels like the street rises up to engulf us like a scene from Hieronymus Bosch. Drug dealers on BMXs career through the crowds on the sidewalk. People huddle in the doorways of abandoned storefronts, smoking crack from glass pipes with long rubber hoses. Others yell at passers-by, chasing down people who owe them money, or cajoling them to lend them some: a buck, a hoot, a taste of it. A shirtless man stands in front of Brandiz's Pizza, half-naked in the yellow-green fluorescent light, jack-knifing at the waist, arms flailing wildly, in the throes of cocaine psychosis, sleep deprivation, or god knows what else. As we walk, I feign a confidence that in fact faded away blocks earlier. I feel overwhelmed, like a tourist in my own neighbourhood. We walk in silence, eyes fixed on the ground. Turning the corner onto Main Street, I remember looking down just in time to avoid stepping in a pool of blood, a syringe lying in the middle.

2.

That night I dreamt of the Downtown Eastside. In the dream, I'm walking down an alleyway with a tour group of middle-class senior citizens. Around us, homeless men and drug users push shopping carts in the dim light of the alley. A wild-haired homeless man climbs atop a dumpster and holds a jewelled crown above his head. Catching the glow from the street lamps above, he projects a beam of light from a gem in the centre

of the crown. Laughing maniacally, he shines the light into the eyes of an old woman from the tour group, blinding her. She covers her eyes, trying to shield them while the man laughs. Her elderly husband ineffectually yells at him to stop. I feel powerless, knowing that the homeless man can do with us what he wants. I run down the alley trying to escape, squeezing through the narrow spaces between dumpsters, stepping over bloody syringes, before finally stumbling into the bright light of the street.

3.

Three years later, near the end of my fieldwork, I found myself in an alley again. It's early morning when I see Dean, one of the panhandlers I've been following, standing in the street. It's below freezing and he's shirtless, his torn and dirty jeans hanging off his hips. He's doubled over at the waist, head hanging down past his knees, swaying from side to side with his arms locked in mock-paralysis. I've never seen him like this before. He's moaning like an animal: deep, long groans that come from the pit of his stomach and echo off the buildings in the still deserted street.

I cross the street and try to talk to him. He's incoherent, but through his tears and slurred speech, I can make out that something has happened between him and his girlfriend. Within a moment, he seems to have forgotten that I'm there and begins moaning again, pigeon-toed and bent at the waist. Just then, the cops show up. The transformation in Dean is incredible: his instincts, when confronted with blue uniforms, are so deeply ingrained that they sober him up in a heartbeat. He tells them I'm a researcher, we're doing an interview, everything's fine. He can only hold it together for a moment though; pretty soon his eyes are rolling back in his head and he's starting to starting to lean again. Thankfully, the cops are satisfied when I tell them I'll walk him home—they're probably grateful to have the problem taken off their hands.

Half a block later, I realize it isn't going to work. Dean walks straight into a telephone pole, bangs his head against it hard and collapses. He's crying, and even though he's barely able to stand, he's lucid enough to blame me for letting it happen. I decide that the best I can do is cut through the alley and get him to the harm reduction clinic. As soon as we turn the corner to the alley though, I'm in over my head again. We're surrounded by people dealing, scoring, smoking rock. A woman calls out to Dean, wanting to know what's up. I try to explain but she pointedly ignores me, repeatedly asking Dean, "Is your friend cool?"

A group of women are sitting on the ground of the freezing alley. "Hey, check out Peanut Butter!" one calls out, taunting my companion with a nickname referring to one of his regular scams. They point and laugh at us as we pass: Dean, slack-jawed and staggering; me, nervous and fumbling. I feel completely adrift, like I've somehow stumbled into the back alley of my dream from years before. I was no more than fifty feet from the streets I walked every day, and yet there in the alley, I was a thousand miles from home.

I begin with these fieldnotes—two stories flanking a dream—in order to pose the two questions that frame this paper: What does it mean to listen for the unconscious in ethnographic fieldwork? And, how should we go about investigating the unconscious dimensions of fieldwork? My argument is that the unconscious is an integral element of qualitative research, especially in methods such as ethnography, which involve long periods of time in the field and close personal relationships with participants. In this paper, I consider one aspect of the unconscious dimensions of fieldwork—the *researcher's* unconscious—and situate this project within the broader projects of *reflexivity* and *positionality* in qualitative research, where researchers attend to the role they play in affecting, and even constituting, their objects of study. I propose that if we are interested in understanding how the unconscious haunts our fieldwork, we would do well to turn to psychoanalysis. This tradition of scholarship offers unparalleled theoretical and methodological resources for social scientists seeking to understand themselves in relation to the field, their research subjects, and their objects of study. This article discusses how psychoanalysis can be used methodologically in qualitative research and presents examples from my own use of psychoanalytic methods. In so doing I hope to demonstrate what attention to the researcher's unconscious looks like in practice, how it might inform ethnographic research, and what challenges the unconscious presents for reflexive researchers.

From reflexivity to the unconscious

The relationships between researchers and their fields have been problematized by many years of post-positivist critique, first under the sign of the "crisis of representation" (Marcus and Fischer, 1986; Clifford and Marcus, 1986; Rabinow, 2007 [1977]; Whitaker, 2010, pages 594–596), and later in feminist geography through the lenses of "positionality" and "reflexivity" (England, 1994; Katz, 1994; Madge, 1993; McDowell, 1992; Merrifield, 1995; Nast, 1994, 1998; Pratt, 2000; Rose, 1997; for an overview see Barnes and Gregory, 1997). These critiques, beginning in the 1960s and reaching their apex in the 1990s, called into question the notion of objectivity in social science research. They demonstrated how the subjectivity of the researcher was an essential component of the research itself, and that the idea that science—especially social science—could be conducted from a neutral, objective "view from nowhere" was worse than illusory, it was ideological. The pretence to objectivity obscured the circumstances of knowledge production, the particular viewpoints it represented, and the power dynamics inherent in research relationships. Feminist scholarship in particular called on researchers to give "greater consideration of the role of the (multiple) 'self', showing how a researcher's positionality (conceived in terms of race, nationality, age, gender, social and economic status, sexuality) may influence the 'data' collected and thus the information that becomes encoded as 'knowledge'" (Madge, 1993, page 295).

While this impulse was certainly an essential corrective to positivist social science, even its earliest proponents foresaw problems with such an approach to reflexivity. In a celebrated article from 1997, Gillian Rose argued that attempts to practice reflexivity in research were hindered by researchers' assumption that they could fully understand their positions as researchers and their relationships to the field, a position she critiques as "transparent reflexivity" (page 305). While the feminist goal of "situating" knowledge is necessary to avoid "the false neutrality and universality of so much academic knowledge", Rose argues that the task of *fully* "tak[ing] account of our own position[s], as well as that of our research participants, and writ[ing] this into our research practice" (page 306) is so overwhelming as to be practically impossible. Because of this, the practice of situating knowledge is often reduced to a truncated form of positionality, where the researcher enumerates a series of social markers—class, gender, ethnicity, privilege—in order to signal her awareness that these markers have influenced the research process. But, as Frosh and Baraitser put it, "declarations of relatively explicit aspects of the researcher's persona will… never be complete enough to understand what her or his contribution to the research might be", because so much of what affects the research we do (and the texts we produce) resists our awareness (2008, page 360). Put another way, it is not enough to recognize that knowledge is situated and partial; what is required is the recognition that "our understanding of the situatedness of our knowledge is *itself* partial, incomplete, ambiguous, and vulnerable" (Barnes and Gregory, 1997, page 21, emphasis added).

This argument—that attempts at reflexivity are hampered by the fact that we are not transparent to ourselves—demands that reflexivity look beyond social identity to those things that remain opaque to conscious reflection. Chief among them, I argue, is the unconscious. The unconscious that I am concerned with is the "dynamic" Freudian-Lacanian unconscious: the psychical system composed of ideas that have been repressed (most commonly, childhood wishes), which subsequently return to consciousness in disguised form, as compromise-formations such as dreams, parapraxes, and symptoms (Freud, 2001 [1900], 2001a [1915]; Laplanche and Pontalis, 1973, page 474). By discussing the unconscious dimensions of ethnographic research, I mean to explore how these repressed ideas return to haunt our research in forms such as dreams, phantasies, our conduct in the field, and the production of research itself.

The unconscious raises different concerns for a project of reflexivity than what was originally imagined by feminism. While feminist reflexivity initially imagined a knowable subject situated within complex fields of power, psychoanalysis posits a split-subject of the unconscious that is defined by its unknowability—by the persistence of an unsymbolizable "kernel" (Lacan, 1998, page 53), which is the "unplumbable… point of contact with the unknown" (Freud, 2001 [1900], page 111). Thus, the reflexivities imagined by feminism and psychoanalysis have fundamentally different objects (Bondi, 2013, page 12). My argument, however, is that one necessarily leads to the other: in

calling on researchers to become reflexive, feminist geography opens a door that must eventually include an analysis of how the unconscious haunts our research just as much as identity and privilege do.

If we accept the premise that the unconscious is a vital component of the reflexive project, the question becomes where to look for it and how to analyze what we find. Jennifer Hunt provides some sense of what this might look like in *Psychoanalytic Aspects of Fieldwork* (1989), which begins with the proposition that much of what occurs in ethnographic research takes place outside conscious awareness. For Hunt, psychoanalysis is a theory of "intrapsychic conflict"—an account of the subject as split between conscious and unconscious, law and desire, one whose "libidinal and aggressive desires are in conflict with the demands of the ego" (page 25). Unconscious desires find their most overt expression in researchers' dreams, fantasies, and parapraxes, but they "can also be disguised more subtly beneath what appears to be rational instrumental action" such as our choice of research topics, methodologies, and how we analyze data (pages 30-32)[1]. In addition, "the unconscious meanings which mediate everyday life are linked to… childhood experiences", which means, in part, that "*transferences*, defined as the imposition of archaic (childhood) images onto present day objects, are a routine feature of most relationships", including the relationships that researchers develop with subjects in the field (page 25). For Hunt, then, psychoanalytic reflexivity attends to the psychic dynamics between researchers and those they research, to the intrapsychic conflicts of the researcher herself, and to possible unconscious motives for many aspects of the research process.

These ideas have been systematized somewhat by the psychosocial studies school in England. Researchers in this emerging field have introduced new qualitative methods at the intersection of psychoanalysis and qualitative fieldwork, including free association and biographical interview methods, alongside a commitment to psychoanalytic reflexivity (Frosh, 2003; Frosh and Baraitser, 2008; Hollway and Jefferson, 2000; Jervis, 2014; Wengraf, 2001). Psychosocial studies proposes that we become "reflexive researchers" who are "engaged in sustained self-reflection on our methods and practice, on our emotional involvement in the research, and on the affective relationship between ourselves and the researched" (Clarke and Hoggett, 2009, page 3). While this psychosocial view of reflexivity includes "unconscious, conflictual forces rather than simply conscious ones", (Hollway and Jefferson, 2000, page 33), Frosh and Baraitser caution us that purely *self*-reflection is insufficient: properly *psychoanalytic* reflexivity must

[1] For research dreams, see Kracke (1981), Levine (1981), and Hunt (1989, Pages 35-37). For an example of parapraxis, see Hunt (page 40). Despite what Katherine Ewing once called anthropology's "almost visceral rejection" of psychoanalysis (1993, page 251), there is a long and fascinating history of psychoanalytic work in the discipline. See Kracke (2012) for a comprehensive review.

necessarily go beyond the "objectification of the researcher" to "objectify the act of objectification" itself (Stepney, 2003, cited in Frosh and Baraitser, 2008, page 359). Put another way, psychoanalytic reflexivity cannot be an entirely self-authored process. Just as one cannot psychoanalyze oneself, psychoanalytic reflexivity requires a position or person beyond the researcher herself..

Within geography, Liz Bondi has recently attempted to respond to this problem through Ronald Britton's concept of the "third position" (Bondi, 2014a). Bondi has long situated her own project of humanistic psychoanalytic geography within the tradition of feminist reflexivity, arguing that "psychoanalytic ideas can be used to support the researcher's use of reflexivity in ways that are sensitive to the power dynamics of interviews [and] that position researchers as witnesses rather than authorities" (2014a, page 45, see also 1999, 2003, 2014b)[2]. Some of Bondi's arguments in this vein—in particular, her commitment to "empathizing" with research subjects (2003)—have prompted critiques that "empathic understanding in qualitative research runs the risk of becoming a form of colonization of the other as the object of research" (Watson, 2009, cited in Bondi, 2014a) because it collapses the alterity of the other into a mere projection of the researcher's experience. Bondi attempts to answer this charge by drawing on Britton, arguing that there are indeed ways that researchers can achieve a sort of reflexive distance from their fieldwork (cf. Jervis, 2009).

Britton's third position is "an unconscious process in which one is not only a subjectively engaged participant in a two-person relationship but also an observer of that relationship" (Bondi, 2014a, page 50, see also 2013, pages 14-15). For Britton, the third position is essentially a developmental concept; it is a product of the successful resolution of the Oedipal situation, in which the child acknowledges the parents' relationship and situates itself as a third party to it. This creates a "triangular space" that makes it possible for the subject to be "a participant in a relationship and observed by a third person as well as being an observer of a relationship between two people" (Britton, 2004, page 47. For Britton, the third position is a capacity that nearly all non-psychotic and non-borderline people possess; it is what allows us to "entertain another point of view, while retaining our own, for reflecting on ourselves whilst being ourselves" (2004, pages 47-48)[3]. Bondi extends this concept to the problem of

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[2] For other considerations of reflexivity within psychoanalytic geography, see Kingsbury, 2010; Oliver, 2003; Pile, 1991, 1993, 1996, 2010; Sibley, 2003; Thomas, 2007, 2010.

[3] Britton uses the concept of the third position to describe the experience of working with psychotic and borderline patients, for whom this third position does not function and who consequently experience any references to an outside of the psychoanalytic dyad (the relationship between analyst and analysand) as traumatic and "threaten[ing to] their very existence" (Britton, 1989, pages 86-87).

reflexivity in fieldwork arguing that this third position is what allows the researcher to "step back" from being "subjectively absorbed in an interviewee's narrative" (2014a, page 50) and gain perspective on the research relationship.

While this approach to reflexivity may appear promising, I do not believe that it resolves the problem of how one can reflect "objectively" on a research relationship or research project itself—not if one wants to take the unconscious dimensions of fieldwork seriously. To help us understand why, we can look to debates within psychoanalysis over the interpretation of the transference, because transference describes a similar intersubjective relation to that which is theorized as reflexivity in qualitative research. Recall that transference is the repetition of earlier relationships within the space of analysis—a common manifestation of which might be the analysand treating the analyst as he or she does the parent of the same gender. A crucial question for psychoanalysis is *how* (and *should*) the analyst bring this dynamic to the analysand's attention—in other words, how can an element of reflexivity be introduced within the relationship. According to the Lacanian analyst Bruce Fink, the problem is that because the interpretation of the transference comes from the analyst, it is experienced by the analysand as coming from the "person she is *imputed* to be by the analysand in the transferential relationship to her" (Fink, 2004, page 6). Within the analytic relationship, there is no way out of this:

> "Just as there is no Other of the Other—that is, no position outside of language that allows us to discuss language as a whole without having to rely on language itself in our discussion—there is no way in which we can step outside the transference situation and invite the analysand to do so with us in order to discuss what is happening in the transference itself" (Fink, 2004, page 6, see also 2007, pages 140-142).

A great many analysts have attempted to resolve this impasse by appealing to a hypothetical place or position beyond the transference from which one could comment objectively on it, from Sterba's "observing ego" and Casement's "internal supervisor", to Britton's "third position" (the former two examples are discussed in Fink, 2007, pages 141–165). Bondi is clearly aware of the problems inherent in this approach when she notes that the process of sustaining the "alterity of the other"—of speaking about the experience of the other—"is always imperfect and faltering", and produces only "an imperfect grasp of what the other feels" (page 50). This prudence is warranted, I think, but the problem is not resolved simply by being more *cautious* when reflecting on the experience of the other or on our unconscious investments in the field.

Rather than attempt to draw distinctions between empathy and identification (Bondi, 2003)—between forms of relating to the other that respect their alterity and forms that "colonize" it—the Lacanian response is to argue that all these relations fall under the rubric of the imaginary—relations between egos—and to insist that the solution lies in

re-situating the relation (between analyst and analysand or between researcher and researched) in the symbolic. To quote Fink again:

> "it is not by *empathically* putting ourselves into another's shoes (assuming that the other wears shoes) that we can determine what to say or do, but rather by working with the other's language and history. Otherwise we are more likely than not to end up deluding ourselves into thinking that we have successfully imagined how the analysand feels and experiences the world, and to cling blindly to our imagined formulation of the world" (Fink, 2007, page 163).

Moreover, it is not simply the *other's* world that we risk occluding by assuming we can empathize with them; we also miss out on an honest encounter with our *own* when we apply this logic to reflecting on our fieldwork. By assuming that it is possible to occupy a "third position", or cultivate an "internal supervisor", we reproduce the conditions that Rose critiqued as transparent reflexivity, thinking that we can objectively assess our own presence in the field.

How, then, should we as researchers work to re-situate our research in the symbolic and find a genuinely reflexive position with respect to our research? How, as I asked at the outset, can we attend to the unconscious dimensions of fieldwork? My proposal is that this question is as much methodological as it is theoretical; it is work best served by psychoanalytic methods that take place through the *medium of the other*—that is, through *speech* that is heard by an*other*.

Three methods

"Only other people can help us see what we ourselves are not yet ready, able, or willing to see" (Fink, 2007, page 165).

In this section and those that follow, I relate my experiences using three closely related psychoanalytic methods: psychoanalytic supervision, case consultation, and psychoanalysis itself. I begin with the experience of analysis itself. If we grant that the researcher's unconscious is an integral component of truly reflexive research, then going into analysis is the most logical way for researchers to begin to understand their own psychical "positionality." This proposal is essentially a direct translation from standard psychoanalytic training, where the cornerstone of an analyst's education is her or his own "training analysis" (Laplanche and Pontalis, 1973, pages 452-454). Only by undergoing analysis themselves do analysts-in-training learn both the technique of analysis and come to recognize their own unconscious when it manifests itself in treatment with analysands. Analysis holds promise for researchers committed to reflexivity because it involves another person—that is, it is not simply *self*-reflection of the sort that Rose and others have cautioned against. Psychoanalysts have long argued that true self-analysis is impossible, for our resistances are the very places where we are most opaque to ourselves. This is why a second party is required, so that the analysand's

speech—with its gaps, slips, and omissions that mark the unconscious—can be heard by an other and put into words.

Translating analysis into a research method is not without its difficulties, however. Free association, which follows Freud's "fundamental rule" that the analysand should say "whatever comes into their heads, even if they think it unimportant or irrelevant or nonsensical… or embarrassing or distressing", (Freud, 2001 [1904], page 251) necessarily charts a long and circuitous path to any insights about research. There is no simple solution to this problem, since any attempt to steer the discussion towards what appears "important" or "relevant" (to say nothing of avoiding that which is distressing) is potentially a manifestation of resistance, attempting to prevent repressed thoughts associated with the research from being articulated. Sceptical readers may be understandably concerned that such directionless free association might lead to researchers simply discussing "their own" problems as opposed to focusing on their fieldwork. While this concern is understandable, in the section that follows I hope to show that the distinction between the researcher's "own" problems and those of the research itself is untenable. Indeed, it is precisely because such distinctions are unstable that psychoanalytic methods are so essential.

Beyond analysis, analytic supervision and case consultation are two promising, and potentially more practical, methods for qualitative researchers—first, because they require less time than analysis (which often occurs three times per week), and second, because they are not so dissimilar from meetings with co-researchers that many are already familiar with. In these techniques, a practicing analyst will see either another analyst (analytic supervision), or a group of analysts (case consultation) to gain new perspective on cases they are working on. Supervision and consultation serve a number of functions for analysts, one of which is better understanding the transference and countertransference dynamics with analysands (Evans, 1996, pages 211–214; Laplanche and Pontalis, 1973, pages 454-461). As I argued above, navigating the transference is difficult because it affects both analyst *and* analysand, such that neither can observe it objectively. Supervision and consultation allow analysts to step outside of this dyad and seek the counsel of someone who can hear the analysand's speech with different ears (Lacan, 2006 [1953], cited in Fink, 2007, page 160n35). This opens up the crucial possibility of *triangulating* the research relationship, that is, to introducing an actual third party (rather than a hypothetical "third position") into the dyadic relation between analyst and analysand, or researcher and researched.

Researchers in psychosocial studies and geography have already adopted these clinical methods in order to overcome the deadlocks of purely *self*-reflexive research (Bingley, 2002; Clarke and Hoggett, 2009, page 17; Frosh and Baraitser, 2008, pages 358-363; Hollway and Jefferson, 2000, pages 130-132; Lendrum, 2014; Marks and Mönnich-Marks, 2003). I have made use of all three methods over the past three years: through my own analysis (beginning January 2012), through case consultation with fellows of the

Chicago Center for Psychoanalysis (2013), and with three clinical supervisors (January-March 2011, September 2012-February 2013, March-November 2013). In the sections that follow, I present two insights to which these methods led me: one in which I discovered how my research was being affected by unconscious conflicts, and one in which a dream revealed the childhood phantasies that subtended my research.[4]

Anxiety in the field

The practice of analytic supervision for ethnographic fieldwork, like conventional psychoanalysis, attempts to create a space where the unconscious can "speak" (Thomas, 2007, page 537). That is to say, it is a space where the researcher puts the experience of fieldwork into speech and has her or his discourse "punctuated" by an other who listens (Fink, 1995, page 66). My experience with psychoanalytic methods began in January-March 2011, when I took part in eight weekly sessions of analytic supervision with a Lacanian psychoanalyst. During these sessions, I discussed my fieldwork and my relationships with the panhandlers I worked with, and I engaged in free association about encounters I had with them. My supervisor asked questions, offered interpretations, and gestured toward connections between the disparate topics that came up.

From the outset, I found the process extremely anxiety-inducing. My anxieties centred on the idea that I would not have enough to talk about. I worried that the bulk of my fieldwork was too far in the past, that I had not been properly immersed in it to begin with, and that analytic supervision would only work if I had been doing "real" ethnography. Moreover, I worried that if I honestly allowed myself to free associate, what would emerge was not what I hoped for—transference dynamics with research participants—but all-too-familiar graduate student anxieties about finishing my dissertation. Simply put, I felt like a fraud and worried that analysis would expose me.

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[4] Cost, time commitment, and ability to access psychoanalysis are issues that will face other researchers who use these methods and these will vary considerably based on geography. Researchers in Sweden may be able to access analysis three times per week and have it paid for entirely by the state while those in the United States may find it unaffordable, or simply unavailable outside urban centers. It is worth noting that many analysts offer a "sliding scale"—in some analytic traditions offering analysis for only nominal fees. In the case of analytic supervision and consultation, my own experience came through a psychoanalytic institute that charged $75 for the year. I can only encourage those interested to investigate the options available to them (See Wengraf, 2001 for the use of supervisory "research panels" in the BNIM method, Hollway and Jefferson, 2000 on "triangulation" as a method, and Clarke and Hoggett, 2009, page 19 for other methods used in psychosocial studies).

Such anxieties are worth paying attention to because, as Freud notes, anxiety is the "universally current coinage" of affects—it is the signal that a thought has been subjected to repression (Freud, 2001 [1917], pages 403-404, 2001b [1915], page 153). Indeed, in the strange and circuitous ways that analysis often works, these anxieties turned out to be very instructive. What emerged in a few short sessions was that my concerns about research and finishing the dissertation were indissolubly tied up in the research itself. A central component of my dissertation was a critique of middle-class discourses about the poor and an impassioned defense of the panhandlers I was working with against those who stigmatized them as lazy. Through free associating about my fieldwork, it became clear that this argument was inextricably tied to my own guilt over laziness with respect to my academic work (cf. Bondi, 2014b, pages 341-342). The obviousness of this connection was only matched by my complete unawareness of it until I articulated it in supervision: I had spent an entire year writing about how panhandlers and drug users were unjustly accused of laziness and "sponging off the government" while all the time I was consumed by guilt that I was not working as hard as I should and that I lived an easy, government-funded life to which I was not entitled.

I returned to this idea years later when I discussed this fieldwork in a case consultation. There, a colleague pointed out that my feelings of guilt implied a relationship of *identification* between myself and the panhandlers I was working with. Identification, for Lacan, is a feature of the imaginary; it is part of the process whereby the ego is constituted through the identification with an image outside of itself (Lacan, 2006 [1949]). My identification operated through the parallels between my position as an early-career researcher and my informants with respect to how we earned money, for all of us relied on the generosity of others to support ourselves: panhandlers by asking passers-by for change and me by petitioning the government for research funds. It was through this unconscious identification that my defense of panhandlers against the charge of laziness also functioned as a way to defend myself against the same thing (c.f. Jackson, 2010, pages 43-44).

Conceiving of this relationship as one of identification helps to explain its deeply *ambivalent* character. As passionate as I was to defend panhandlers against charges of laziness, I frequently found myself angry with them when they acted in ways that seemed to confirm people's worst suspicions. Why *did* they always have to hit me up for money? Why *couldn't* they just get jobs? Why did we have to go through the same song and dance every day about why they needed money when we both knew they just wanted to get high? Despite disagreeing with these arguments when others made them, and despite knowing that there were real structural reasons behind their poverty, I frequently found myself caught up in these reactionary discourses. The reason, I believe, is because imaginary identifications are always marked by ambivalence. I looked to these panhandlers for confirmation that I was a responsible researcher, loved them when they confirmed this, and hated them when they failed to. Rivalry and aggressivity are the

necessary corollary of love and empathy in the imaginary—they are the other side of the coin of identification, or in properly Lacanian terms, the other side of the Möbius strip, to which one passes without ever leaving the first surface (Blum and Secor, 2011; Kingsbury, 2007).

What are we to make of these unconscious connections and identifications, though? A reviewer of an early draft of this paper expressed concern with the parallels that I am drawing between the guilt that graduate students experience over their work ethic and the accusations of laziness that are levelled at panhandlers. The reviewer argued that it is politically irresponsible to conflate what are clearly distinct phenomena, borne out of radically different discourses and crosscut by class, race, and the history of middle-class attitudes towards the poor. This is undeniably true: there is a vast gulf between these two forms of guilt and the discourses that produce them. I want to argue, however, that historicist arguments such as these miss a crucial point, which is that *at the level of the unconscious*, there is potentially no difference between these things at all.[5] Freud argues that the Id has no sense of time, value, or morality (Freud, 2001a [1915], page 187n1, 2001 [1920], page 28, 2001 [1932], page 74), so we should not be surprised if our unconscious associations are politically problematic or improper. Moreover, ignoring these sort of unconscious associations can have very real effects—as I learned, my guilt-ridden identification with panhandlers turned out to have significant intellectual consequences for my research. As a result of my unconscious attempts to defend myself against the guilt I felt over laziness, I focused on one aspect of the politics of panhandling—middle-class discourses about poor people's morality and industry—at the expense of others, such as a more thorough consideration of the structural forces that produce poverty. This is consequential because there are good reasons to critique research for remaining within the categories of middle-class morality. As Loïc Wacquant has argued, assessing poor people's conduct in terms of middle-class norms is highly problematic, even in research like my own, which attempted to defend panhandlers *against* charges of laziness by "invert[ing] its valence, turning a negative into a positive" (2002, page 1481). Despite the inversion, such research is nevertheless still confined within a moral universe that might itself be criticized. To the extent that I was myself struggling with that world, and defending my informants on its terms in an unconscious effort to exonerate myself, my research could be said to have remained blind to other, potentially more salient issues, such as the structural forces identified by Wacquant (cf. Hunt, 1989, page 41).

The fluidity by which associations are made in the unconscious is one of the reasons why it remains a problematic object for many social scientists (Callard, 2003). Even today, when self-reflexive researchers no longer aspire to detached objectivity, taking

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[5] See Kingsbury (2007, 2014) and Zizek (1989, page 51) for similar psychoanalytic critiques of historicism.

the unconscious seriously means that our motives are not only *not transparent* to ourselves, but may in fact be motives we find politically problematic or even shameful. Through analytic supervision, I discovered that a significant argument in my dissertation was a product of the guilt that I wrestled with during its writing, and how I was completely unaware of its influence on my work until I began to free associate about my research in supervision. In the section that follows, I return to the dream that I recounted from the beginning of my fieldwork and—using insights gleaned from my own analysis— examine some of the phantasies that haunted my research from the outset. These phantasies, like the guilt I have just described, proved to have significant effects on my research.

Research phantasies

In *The Interpretation of Dreams* (2001 [1900]), Freud draws his well-known distinction between the manifest and latent content of dreams. The manifest content are the images and narratives that we recall from our dreams. The latent content, meanwhile, are the repressed thoughts that give rise to these images, which dream interpretation seeks to elaborate. Freud argues that dreams take their manifest content from what he calls the "residue of the day", (2001 [1912], page 265), that is, from the memories of the previous day (Freud, 2001 [1900], 165-188). This residue is formed into dream images through a process of *condensation,* whereby recent thoughts and memories are combined with earlier memories. Through this *dream-work*, overdetermined dream images emerge, connecting the events of the day with enduring memories such as those from childhood (pages 277-309).

In my dream of the alleyway, we can see clearly how the dream draws on the residue of the day: it is set in the Downtown Eastside and features homeless people, as well as the syringe that I nearly stepped on. When I set out to analyze this dream, the image that I found myself drawn to was that of the laughing homeless man holding the jewelled crown aloft, shining the light into the old woman's frightened eyes. During analysis, I free associated on this image, trying to trace its provenance. I soon remembered an image of Moses holding the tablets of the Ten Commandments.
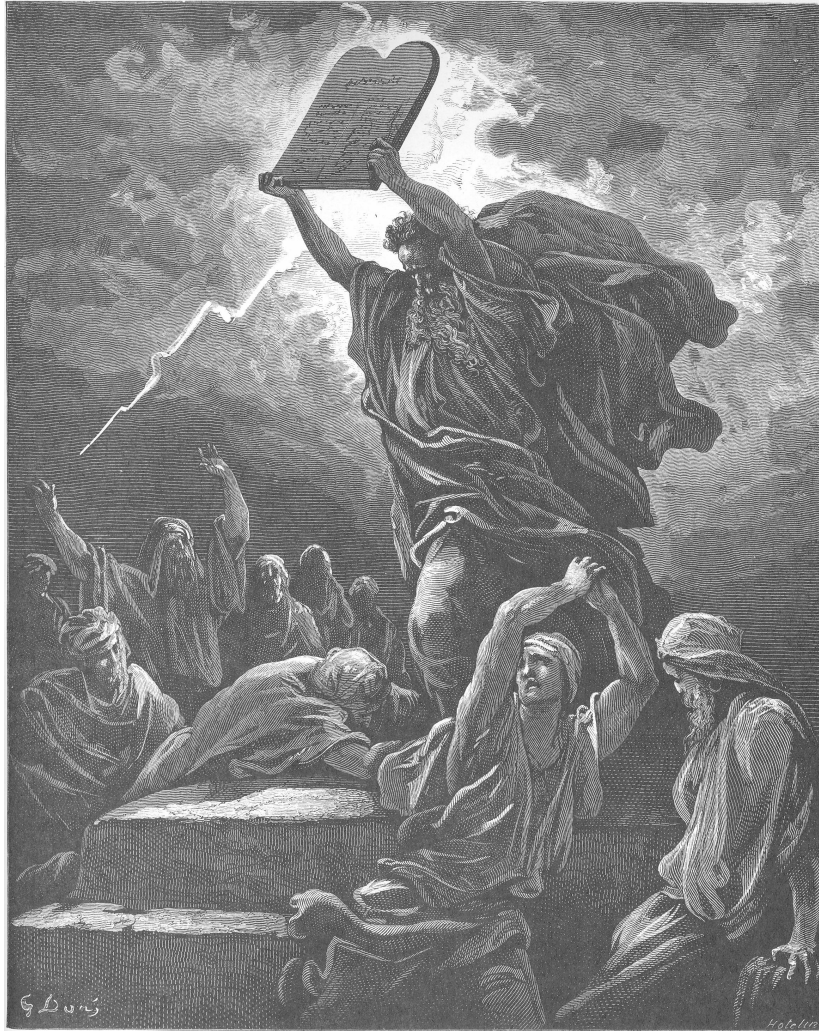
Figure 1. *Moses Breaks the Tables of the Law.* (Engraving by Gustave Doré, 1865).

The association seemed clear: both figures were bearded, had wild hair, and held objects above their heads, shining beams of light at people below. When I encountered this image, however, as well as numerous variants of it, I was unsatisfied. Nothing about Moses or the Ten Commandments generated further associations or connected to other thoughts that I had about the dream. It was only during further free association that I realized that the connection I had drawn was only partial: the image of Moses was an intermediate step in a process of metonymic substitution. The image that followed in the chain of associations confirmed Freud's argument that alongside the residue of the day, it is memories from childhood that are most commonly cathected in dream symbols. I realized that the image of Moses concealed a far more significant memory from my childhood: Indiana Jones.
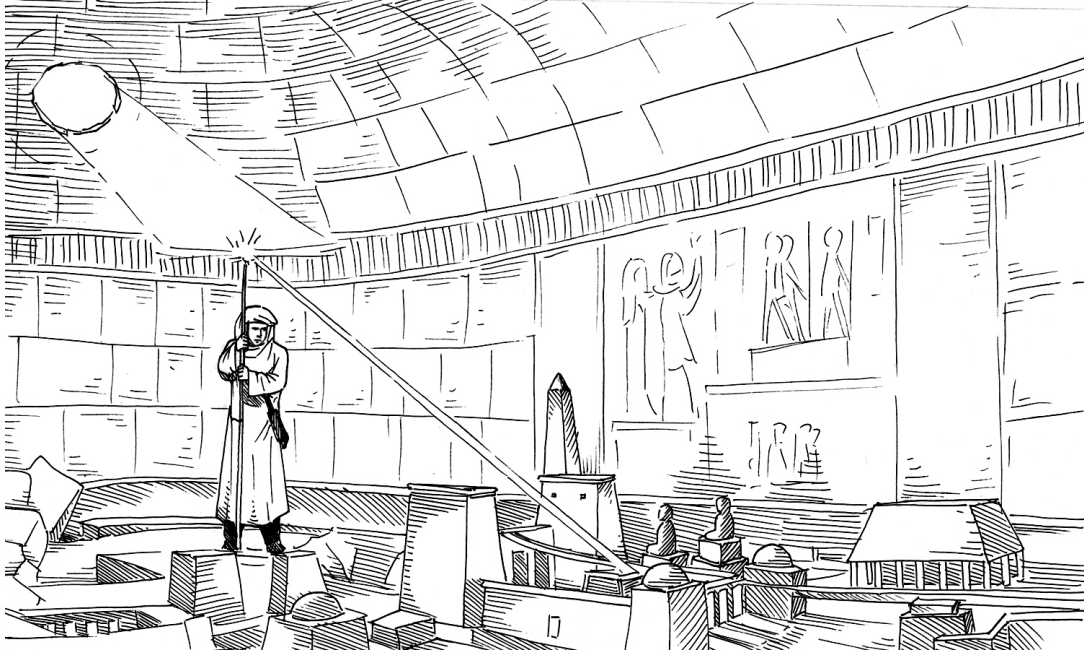
Figure 2. Indiana Jones illuminates the location of the Ark of the Covenant. From *Raiders of the Lost Ark* (directed by Steven Spielberg, 1981). Illustration by Garyl Araneta.

In this scene from *Raiders of the Lost Ark*, Indiana Jones shines a beam of light at a relief map from a red gem mounted on a staff. The connections to Moses are numerous: both figures cast beams of light, come from the desert, and are associated with the Ark of the Covenant, which was both the place where the tablets of the Ten Commandments were stored and the object of Indiana Jones's quest in the film *Raiders of the Lost Ark*. Moses here functions as a screen, obscuring (and overdetermining) the latent dream thought, which would have revealed the association too blatantly (Freud, 2001 [1901], page 672).

The image of Indiana Jones is a potent one from my youth and immediately calls to mind childhood desires for adventure and exploration, desires that I would now characterize as quasi-colonial. I believe that this issue is central to the dream's latent content and the means of its representation. When we consider this theme of colonial adventure and combine it with the trope of being part of a senior citizen's tour group, we can read the dream as a distorted expression of a phantasy of being the masterful explorer, the heroic man who moves freely through the dark and dangerous places of the city (Hunt, 1989, pages 30-31; cf. Razack, 2000, Page 113).

Reflecting on the dream, I felt an overwhelming sense of embarrassment at the day that preceded it: taking my advisor down Hastings Street was very much an act of tourism (note the *tour group*), and it was strongly inflected by a desire to show off an exotic place and demonstrate my mastery over it. This embarrassment, and the anxiety that came from being overwhelmed by the intensity of the Downtown Eastside that night, is reflected in the dream by the role of *light*. In playing the role of researcher, I saw myself as "shedding light" on the "dark places" of the Downtown Eastside. And yet, here in

the dream, light does not illuminate, it *blinds*. Whereas Indiana Jones' light points the way to the location of the Ark (to Truth), in my dream, the light shining from the homeless man's crown blinds the people in the crowd. My embarrassment over these desires introduces a further distortion into the dream by inverting my identification with Indiana Jones, placing the homeless man in the central role and casting me as the subordinate. The crown he holds makes him the king of this place; he is the one in charge, while I am just another hapless tourist, blinded and lost in the dim light of the alley.

The dream thus speaks to some of the unconscious phantasies animating my research: phantasies of adventure, exploration, and the exotic, as well as anxieties about my inability to navigate these foreign places and the frightening figures who live there. Such phantasies are not easy to admit because they are obviously politically problematic. They certainly do not sit well with what I *consciously* wanted to accomplish with my research: contributing to better understandings of drug users and panhandlers through engaged ethnographic fieldwork and advocating for stigmatized neighbourhoods like the Downtown Eastside. Problematic or not, at the level of the unconscious, these phantasies are quite real and coming to terms with them is an essential part of conducting genuinely reflexive fieldwork.

It is a testament to the discomfort I still feel over this dream that in an earlier draft of this article I characterized it as an "unconscious auto-critique", that ridiculed my desire to be the colonial explorer. One of the reviewers was sage enough to point out that the unconscious does not critique, and my contention that it was doing so was "dangerously close to subjecting [my] own unconscious phantasies to moral scrutiny and judgment". This is quite true. I would now argue that reading the dream as a critique is itself a wish—a desire to distance myself from uncomfortable phantasies. This guilt, and the actions I have taken to defend myself against it, offer further illustration of what psychoanalysis contributes to the project of reflexivity: if Kobayashi (1994) argued that reflexivity "must be a process of self-critique" (cited in Rose, 1997, page 313), psychoanalytic reflexivity pushes us to think about how our self-critiques can sometimes themselves serve unconscious goals by preventing us from doing other forms of reflexive work.

Let me conclude by asking, once again, what precisely should one *do* with these sorts of psychoanalytically reflexive revelations? Through analysis, I learned that my moral defense of panhandlers emerged in part out of my own guilt and that my attempts to portray my informants in positive terms were subtended by phantasies about them that were clearly quite ambivalent. One response could be that these revelations undermine the research and suggest that I should have ignored the issue of morality entirely. I would like to suggest, however, that rather than discarding research findings based on the revelations of psychoanalytic reflexivity, a more sophisticated approach is to treat these revelations *dialectically*, by folding them into one's earlier findings. In this way, the

discoveries of reflexivity can be brought back into the field and into our writing, making unconscious associations into yet another piece of data that we must analyze. To take my own research as an example, this dialectical process leads us to a more nuanced way of thinking about the status of morality in research on poverty. If I began by offering a defense of panhandlers against charges of laziness because I unconsciously identified with then and was defending myself against that same charge, and then, by recognizing this through analytic supervision, realized that this blinded me to potentially more important structural issues, then the dialectical manoeuvre is to fold this reflexive moment into the first, producing what I hope is a more sophisticated way of understanding how and why morality continues to animate discourses of poverty.

Conclusion

"Fieldwork is, in part, the discovery of the self through the detour of the other" (Hunt, 1989, page 42).

"What we research is our relation with the researched" (England, 1994, page 86).

My goal in this article has been to offer an answer to the question of what it means to listen for the researcher's unconscious and to explore methods for doing so. As I hope to have demonstrated, psychoanalytic methods can lead to insights about how the unconscious plays out in the field, how we relate to our informants not simply as participants or sources of information but also as phantasy objects, and how our anxieties and phantasies can end up constituting the very questions that we ask—and conclusions that we draw—in our research.

In a recent critique, David Graeber (2014) has argued that our enduring fascination with reflexivity should be understood as an artifact of rise of the neoliberal "professional-managerial class" in academia, which has promoted "a kind of constant nervous self-examination" (page 84) while simultaneously effacing any potential for this reflexivity to change existing power relations.

For Graeber, the crucial distinction is one of "*being* reflexive", meaning *self*-reflection, and "actually *doing* reflexivity", which involves continually reflecting on the power dynamics of fieldwork in dialogue with those whom we research (page 83). Simply *being* reflexive can easily remain trapped within a "confessional mode of moral self-narration", where members of the academic elite meditate on the "minutiae of [their] own power and privilege" and "compete with one another for moral superiority based on claims of greater cognizance of their own compromised nature" (pages 82–83). While I have made a significant effort in this article to distinguish this sort of self-

reflection[6] from psychoanalytic methods that aim to decentre the 'self', Graeber's critique may well still apply to the psychoanalytic methods I have described here. As we continue to develop psychoanalytic reflexivity in the discipline—expanding its scope from the researcher's unconscious to include the unconscious dimensions of our research *relationships* (e.g. transference between researcher and researched), the unconscious of our research participants, and the broader "libidinal economy" of social formations (Hook, 2008)—what is crucial is the dialectical maneuver that I have described in this article: we begin to truly *act* reflexively once we take what we have learned through psychoanalytic reflexivity and fold it back into our research practice and our texts, allowing it to reframe our research, open us to new ways of interpreting our data, and potentially to change our relationships with those with whom we work.

Psychoanalytic reflexivity undoubtedly raises thorny questions for qualitative research. The specter of what Felicity Callard calls the "truly monstrous" revelations of psychoanalysis looms over any attempt to take the Freudian unconscious seriously (2003, page 308). What are we to do with the *jouissance* of the researcher—with his or her pursuit of an impossible enjoyment that may conflict with what the research, or research subjects, demand? Or, what are we to do when we discover in our research currents of narcissistic introspection conducted "through the detour of the other" (Hunt, 1989, page 42)? How, finally, do we reconcile the subjective truth of the researcher, in all its ugliness, with the ethical imperative to use our research in service of the good (c.f. Lacan, 1992)? As I have argued, my own psychical conflicts played themselves out in my research in at least two ways: first when I unconsciously used my research as a venue for working through anxieties and guilt over laziness, and second, in my dreams, as a way to both indulge and defend myself from phantasies of being the brave explorer of dangerous and exotic places. There is a strong temptation, when confronted with such embarrassing and unpalatable revelations as these, to castigate psychoanalysis for paying attention to them—to characterize them as distractions from the *legitimate* things we are studying, like the very real lives of those whom we research. The problem is that if we follow the logic of reflexivity through to its end, we must grant that the unconscious affects our research as much as identity and privilege do. What psychoanalysis offers us is the possibility of a more honest and ethical relationship to the "monstrous" revelations of the unconscious. Rather than haunting our research as absent presences or repressed signifiers that unconsciously determine our conduct and our arguments, psychoanalytic methods bring our monsters into speech and into our texts where they can finally be heard and reckoned with.

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[6] In Lacanian terms, the sort of reflexivity (moralizing, competitive, guilt-ridden) that Graeber critiques is a thoroughly *imaginary* phenomenon that would need to be submitted to analysis.

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